Dear Editor,

I send you herewith our responses for G. Giordano's review of our paper:

"The strength and permeability of tuffisite-bearing andesite in volcanic conduits"

By S. Kolzenburg, M.J. Heap, Y. Lavallée, J.K. Russell, P.G. Meredith and D.B. Dingwell

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The reviewer's comments raised several interesting questions which, in our opinion, have significantly strengthened the content and thus the message of our study. Based on G. Giordano's constructive comments, as well as those of reviewer #2 (Kathy Cashman), we have slightly reformulated the angle of our manuscript and as such wish to modify the title of our study to "Strength and permeability recovery of tuffisite-bearing andesite" as this better portrays the essence of our findings. Our replies to his comments, and the suggested changes to our manuscript, are outlined below. The original reviewer comments are in blue, our replies are in black italic, and our suggested changes to the original manuscript are in black.

"The term tuffisite is highly ambiguous; it is used in kimberlite diatremes and maar literature to define almost whatever pyroclastic deposit filling the pipes; Tuffen et al 2003 used it for veins formed internally to a rhyolite, capable to be healed with time; it is used as a synonym to intrusive pyroclastic- or just clastic-filling cracks formed at fragmentation level or at magma-water interaction level etc etc.."

We agree that the term "tuffisite" has been widely (and sometimes disparately) used in the literature. The rock name was first introduced by Cloos (1941) as: "veins comprising lithified mixtures of particles of juvenile volcanic material interspersed and intimately mixed with host rock fragments of similar size". However, it has been somewhat misused, especially in the kimberlite literature, and applied to diatreme-filling, fragmental material. We presume that this misinterpretation is related to the fact that the original paper was published in German, which may have led to translation errors. The commonly accepted interpretation of the generation of tuffisite is as follows: (1) subsurface fragmentation of volcanic material; subsequent (2) mobilization and (3) re-deposition of such fragmental material by flow of a gas-particle suspension through fractures in the volcanic edifice (Heiken et al., 1988; Stasiuk et al., 1996; Tuffen et al., 2003). Our samples derive from rocks that host volcaniclastic material in veins and sheets. Petrographic analysis has shown these veins to be polymictic and comprising a *mixture of juvenile and accessory material. We therefore think that the term tuffisite serves as an accurate description of the rocks used herein.*

This ambiguity then is reflected on the interpretation of your data, because it is not clear at all from the paper what is the author's interpretation of the juvenile-free, crystal-fragments that form the veins. Did they form at the time of the andesite domes (so why no glass)? Did they form as clastic dykes during some later phreatic event? Did they for at the time of the 2005 eruption (so why no glass)?

Petrographic descriptions and the rheological assessment of the dome samples from Volcán de Colima have shown that most of the rocks contain interstitial glass, but some samples are holocrystalline (Lavallée et al., 2012b). Yet, looking at our initial manuscript anew, we realize that our original sample description was lacking detail as to the origin of the samples. The fragments present in the tuffisite veins appear to have a more restricted grain size than the crystal-size distribution observed in the host rock, a fact we will now mention in our revised text. In thinking about the potential mechanical reason for such an observation, it is not so difficult to envisage that a rock formed from the processes of fragmentation, transport, emplacement and subsequent solidification experienced a certain degree of particle differentiation. In such a likely case, one would expect the finest fraction (i.e., fragmented glass shards from the groundmass) to travel further through the permeable network. We would like to add the following statements to the sample description in order to clarify this issue:

The rocks described here were collected as loose blocks selected from the pyroclastic flows of the 2005 explosive activity. Thus their exact origin in the rapidly ascending magma column remains a subject of uncertainty. Thin sections were prepared perpendicular to the long axis of the veins and orthogonal to each other.

And:

The tuffisites tested in this study are holocrystalline and consist of coherent fragmental material. The crystal size within the veins generally differs from the host rock. Large plagioclase and pyroxene phenocrysts generally appear broken several folds within the veins. Iron oxides are sometimes larger in the veins than in the host rocks, suggesting that they have been transported from another area (with a different petrographic equilibrium). The contact between the host rock and the veins is generally irregular.

Moreover, we wish to add that in the time between submission of this manuscript and resubmission of our manuscript there was a joint field campaign between the Ludwig-Maximilians University and the University of Colima that surveyed the last lava dome in *the crater of Volcán de Colima. During this field campaign, a large number of tuffisite sheets and veins were documented within the growing lava dome, which was seen as strong supportive evidence for tuffisites that were formed as a result of the weaker, non-destructive Vulcanian eruptions (Lavallée et al., article in preparation), observed 5-25 times/day during period of dome growth (Lavallée et al., 2012b).*

Note that the entire discussion on the transient role of these veins in terms of permeability and strength, due to possible processes of veins healing is appropriate only for the first case. I strongly suggest the authors to first dismiss the term tuffisite, then define exactly what type of clastic/pyroclastic veins have been sampled.

Once again, following our aforementioned arguments where we assess the structures indicative of tuffisites, we insist on keeping this nomenclature. There is an unfortunate lack of description of tuffisites in the volcanological literature; certainly tuffisites do not out-crop everywhere and their formation process, sourced in fragmentation, may well underline their weakness and rare preservation. As such, the physical characterization provided in our study is seminal in advancing our understanding of conduit processes, and especially in quantifying the possible strength and permeability of the rocks in conduit environments. The tuffisite-bearing andesites we found, however, were selected from the pyroclastic flow deposits emplaced during the 2005 explosive activity and therefore we are unable to pinpoint their exact origin and orientation in the magma column. A more in depth discussion of the strength- and permeability-depth distribution in the conduit is not possible. Within such bounds, we restrict our discussion to the possible mechanics acting within conduit areas.

Only after that it will be possible to give an appropriate interpretation to the role of those veins in terms of mechanics and permeability. For example, do the authors think that the presence of those veins played any role in the dynamics of the 2005 eruption (or pre-eruption)?

This point raised by the reviewer is indeed fundamental to an understanding of the role of tuffisites on volcanic activity. However, it is a point that cannot be solved by our mechanical study. The point refers to whether the formation of tuffisites through local fragmentation would affect the energy budget stored in the pore space and mitigate the potential explosivity of a magma column. In a recent article, (Castro et al., 2012) demonstrated that tuffisite veins preserve a gradient in volatiles, decreasing towards the veins, which may indicate the capability of tuffisitic areas to degas; yet, they conclude that such local degassing effects would not be sufficient to doctor the energy stored in the column as a whole. They also bring their discussion one step further by questioning whether the event of fragmentation itself, and the network of the tuffisites, might have *been permeable enough to degas a larger region inside the magma column. This is where our study, although on different rocks (andesite vs. obsidian), comes into play. Our study cannot assess whether the formation of the tuffisite veins influences the eruption behavior. Yet it is a fact that the frequent (5-25 times/day), small Vulcanian eruptions were degassing the lava dome during growth periods. If each event was accompanied by the formation of tuffisites, then it is possible that they acted on the longer time-scale behavior of the dome; but this is speculative and our data do not help constrain this sequence of logic. What we may say, however, is that the tuffisites have recovered their physical properties to a state that is comparable to the host andesite, suggesting that the influence of tuffisites on lava dome activity will be restricted to the time necessary for strength and permeability recovery, which we propose to be relatively short. Furthermore, the fact that tuffisites were found within the core of blocks emplaced in a pyroclastic density current deposit shows that they were strong and that they did not mechanically influence the propagation of fractures during the fragmentation of the lava dome. We would like to expand the discussion in order to address the influence of tuffisite veins on eruption dynamics and slope stability, as outlined at the end of this rebuttal letter.*

The paper discusses longly about the time needed for veins to recover (reduce porosity and increase mechanical strength). However while a series of potential processes for recovery are discussed it is not clear which one is that relevant for the Colima samples.

Petrographic analysis shows the veins to be devoid of significant amounts of gas phase precipitates, which could have played a role in the solidification of the tuffisite veins. Welding of juvenile vitric materials could have played a role. However, the matrix of these rocks is holocrystalline and mainly composed of crystals fragments with rare intact microlites; devitrification could have occurred after welding and lithification or it may have operated before welding, in which case an alternative lithification process is required (i.e., hot pressing). Thus, we cannot rule out welding as a solidification mechanism in the rocks, but there is a lack of definitive evidence for the process. A third mechanism, which is likely to be the most important reason for differentiation and depletion of glass within the rock, is that degassing potentially transported fine-ash particles further than the coarser crystals left to build the tuffisites we collected. Needle-shaped microlites are sparse in the tuffisite matrix, yet they are abundant in the host rock. Independent of which of these processes is involved (and dominating) in the creation of the rocks tested in this study, the statements made about post solidification strength and permeability remain.

I would actually like to see more discussion about the role of these veins on the bulk mechanical strength of the shallow volcanic system in terms of potential for slope failure.

This is certainly an interesting and important question. The presence of tuffisites may have a wide range of effects (both positive and negative) on edifice stability and the potential for slope failure. Any statement towards stabilizing or destabilizing effects would need to be tied to the spatial distribution of tuffisite within the volcanic edifice. As we do not have such information, and are unaware of studies reporting on the spatial and geometrical distribution of such veins, we feel unable to provide more than speculations on their potential to influence slope stability. The same implications discussed with respect to the contribution to degassing of magma (dependence of permeability and rock strength on the state and rate of healing) also apply to slope stability, only that here they are much more intimately linked to the distribution and orientation of the veins. In the "fresh" state, where tuffisite veins are weaker than the host rock, they may destabilize their surrounding and favor slope failure. However, if they reach a recovered state, where they become relatively strong, they may in fact strengthen the flanks of volcanoes as they would act as a strong and rigid structure introduced into an environment that is inherently fractured. We greatly appreciate the invitation to discuss these issues in more detail and would like to expand the discussion with the addition of the following paragraph:

Tuffisite production within a volcano might nevertheless have wider implications for the eruptive behavior and the edifice structural stability. During fragmentation and production of the tuffisitic material, stress is released through fracturing (instead of allowing for a build up of stress leading to eruption), which in turn allows gases to escape (Castro et al., 2012), at least for the time it takes to recover host rock values of permeability. In this manner, the formation of tuffisites may actually retard an eruption and perhaps lessen its explosivity. The same implications discussed with respect to the contribution to degassing of magma (dependence of permeability and rock strength on the state and rate of healing) also apply to slope stability only that here they are much more intimately linked to the distribution and orientation of the veins within the volcanic edifice. In the "fresh" state, where tuffisite veins are weaker than the host rock, they would presumably be destabilizing and might engender slope failure. If however they reach a healed state, where they become relatively strong, tuffisites may in fact strengthen the flanks of volcanoes as they would act as a strong and rigid structure introduced into an environment that is inherently fractured.

Kind Regards,

S. Kolzenburg and co-authors

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